THE HISTORY

OF

SPONTANEOUS GENERATION

BY

EDWARD S. DUNSTER, M D.

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A PAPER

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BY

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BY EDWARD S. DUNSTER, M. D.

In connection with the very able paper of Dr. Lionel Beale, on the nature of life, read at our last meeting, it has occurred to me that a historical sketch of the rise, progress, and present status of the theory of spontaneous generation might be of value. We cannot approach the study of the wonderful mystery we call life without coming, at the very outset, face to face, with the problem of its spontaneous origin; and we must examine and either set aside or accept it, before we can make headway with the higher questions involved in such study. It is instructive, also, for us as students of science to occasionally survey the past, and observe the slow approaches by which our present knowledge has been attained. It gives us an insight into the character of our work, and compels a higher appreciation of its positiveness, when we see that it has been gathered literally by centuries of patient and cautious investigation, in the process of which error after error has been eliminated; and thus, steadily though very slowly, there is a nearer approach to ultimate truth. Such a retrospect may well serve to restrain the impatience of those who are disposed to scoff at science by reason of its changing phases, for it is the distinguishing characteristic of true science that she does not let a belief or theory encumber her progress when fuller investigation has shown that such belief or theory is no longer tenable, but she sweeps it away as remorselessly as the whirlwind crushes down the forest in its advancing track, and rejoices

that "the grave of each superstition which it lays is the womb of a better birth." *

I do not purpose, however, to enter into a discussion of the arguments for or against the doctrine of spontaneous generation. Such a task would require a series of lectures, instead of the limited time allotted to me this evening, and it is doubtful, too, if the resulting gain would be at all commensurate with the labor, for it is a question which in the nature of things cannot be argued on the grounds of authority or of probability, but must rest on experimental evidence alone. In the interpretation of this evidence, however, we may with propriety accept the opinions of those who by long training in scientific research are best qualified to estimate such evidence at its real value.

The history of the doctrine of spontaneous generation may be conveniently divided into three epochs. The 1st covers the period from Aristotle, 325 B. C., to Redi, A. D. 1668. During this epoch spontaneous generation was believed by all naturalists to be the common mode of the production of a very large class of animals. The 2d epoch extends from the time of Redi to the experiments of Schwann and Schultze, in 1836–7. This epoch presents two phases, one relating to the generation of animals visible to the naked eye, the other relating to the generation of infusorial animalcules invisible to the unaided eye. As regards the first, spontaneous generation during this epoch "was narrowed down to a rare and exceptional mode of the reproduction of a few only of the most obscure species, and finally shown to be untenable even for them." † The generation of a large share of the entozoa was also explained during this period, and they were removed from the class formerly believed to be produced by spontaneous generation. As regards the other phase of this epoch, that relating to the infusoria, it may be said that

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* MAUDSLEY: *Body and Mind*; London, 1870, p. 111.

† J. C. DALTON, M. D.: *Spontaneous Generation*, *New York Medical Journal*, February, 1872. I am indebted to this admirable paper of Prof. Dalton's for much of the material here made use of, and I desire now to express my acknowledgments for such use in the instances where I have not given the reference and page.

the mode of their generation was quite fully explained, at least for all but the very lowest species, and that generally scientific men held that the question was put to rest by the decisive experiments of Schultze and Schwann, just alluded to. The 3d epoch dates from the year 1858, when the question was reopened in Paris for special reasons connected with the study and theory of evolution. In this, the present epoch, the whole battle ground is within the domain of infusorial life, and although, by the majority of scientists, the victory thus far is conceded to the advocates of biogenesis, or life from preëxisting life, a few still courageously contend for the opposing theory—abiogenesis—life without preëxisting life, or life from inorganic matter alone.

1*st Epoch.* During this period a belief in the spontaneous generation of many animals was universal. Even Aristotle, who may be considered to represent the highest type of the scientific culture of the day, divided animals, with reference to their mode of production, into two classes. The one was derived by succession from preëxisting parents, life being transmitted either by the production of living young resembling the parents, or through the hatching of eggs, or, as in many insects, by grubs or larvæ. In the other class no such connection could be traced, and hence they were considered to originate spontaneously from "the fortuitous concourse" of inorganic materials, from the slime or ooze at the bottom of the sea, or from the decomposing remains of other animals. Thus the shell-fish, such as clams and oysters; the sea-nettles and sponges; the maggots that invariably swarm after a time in dead meat; very many of the smaller insects that appear so suddenly; grubs, moths, eels and many other small fishes, are enumerated as originating in this way. The idea of decomposition and recomposition of organic atoms was a favorite one, not only during antiquity, but down through the middle ages. It finds its best expression, perhaps, in Aristotle's well known formula, *Corruptio unius est generatio alterius.* These crude beliefs were not confined to the Greek scientists alone, but continued even down to the middle of the seventeenth century, at which late day Kircher, the learned Jesuit, declared that to

produce a crop of serpents it is only necessary to pulverize one and sow the powder as seed in the earth. He further averred that fragments of plants falling into water became transformed into animals, and he actually figured such animals in his book.* Van Helmont, too, we find describing a mode for the artificial propagation of mice, frogs, and eels.

With our present knowledge of the mode of reproduction in animals, we may perhaps smile at these crude and incorrect notions, but we must remember that they were the best conclusions then attainable, and they were the result of a truly scientific but imperfect study of natural phenomena. Dr. Dalton well says: "Aristotle represented in natural science, as in so many other departments, the entire scope and successful activity of the Grecian intellect. He occupied the position which was afterward held by the Buffons, Linnæus, and the Cuviers of more modern periods; and it is certain that the opinions which he expressed must have seemed reasonable from his point of view."

It is not out of place to mention here some of the causes of error which are now apparent. The young of many of the lower varieties of animals are so wholly unlike the parent, that it was impossible to trace any similarity or relation between them, until after patient observation the intermediate stages in their development were learned. A familiar illustration of this is the larval form of the common butterflies and moths, and the varied appearances seen in alternate generation in insects. In their successive developmental stages the animals will often inhabit different localities, and in some instances even different elements. The secretive habits of many of the oviparous and viviparous animals precluded for a long time knowledge of their mode of reproduction. Some, as for instance fishes, will migrate long distances, deposit their eggs quickly, and as suddenly disappear. After a time the ova are hatched by the favoring influences of light and heat, no parent animals being present in the vicinity. In others the young on being hatched quickly betake themselves to a different locality. Again, ova not infrequently lie dormant, as it were,

* *Edinburgh Review*, Vol. 89, p. 167.

for a season, or even for a period of years, and when finally developed the parent animals have long since disappeared from the face of the earth, and hence an easy belief in a new or spontaneous generation.* Gradually, however, after years and even centuries of patient investigation, all these and the kindred difficulties have been removed and the errors have been explained, so that even during this first epoch some of the supposed cases of spontaneous generation were removed from this category, and explained in accordance with the increased light thus gained. This brief survey of the first epoch in the history of spontaneous generation must suffice. Indeed, thus much of reference to it is only pardonable, for the purpose of contrasting the opinions then prevailing among scientific men, with the more positive knowledge which now obtains.

2d Epoch. The first solid and experimental advance toward the positive knowledge of to-day, and the first distinct repudiation of the doctrine of spontaneous generation, was made in 1668, by Francesco Redi, the Italian naturalist.† "He did not trouble himself," says Huxley,** "with speculative considerations, but attacked experimentally what had been considered to be particular cases of spontaneous generation." He directed his attention first to studying the origin of maggots in putrefying meat. He observed that before the appearance of such maggots flies were invariably to be seen hovering about and alighting

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* "A remarkable instance of this is the case of the American seventeen-year locust (*Cicada septendecim*), where a period of seventeen years elapses between the hatching of the larva and the appearance of the perfect insect; the larva all this time remaining buried in the ground, while the life of the insect in its perfect state does not last over six weeks. A brood of these locusts appeared in the city of New York and its immediate vicinity in 1843, and again in 1860. If they return with their accustomed regularity, their next appearance will be in 1877."—DALTON: *loc. cit.*

† FRANCESCO: An Italian physician, 1626–1697, distinguished alike for his attainments in literature and in natural history. His writings have been collected and published in a single volume. *Opuscoli di Storia Naturale.* Florence, 1858.

** *President's Address to British Association*, at Liverpool, Sept., 1870. This address has been published in many journals, and also in separate form. *Nature*, Sept. 15, 1870, p. 400.

upon the meat, and he suspected that they were the progenitors of the maggots. In midsummer he took a number of wide-mouthed jars, and placed in them bits of flesh. Some of the jars were left open—some were covered with paper carefully secured around the neck. Maggots soon appeared in the open jars, but none were seen in the closed jars, even after weeks had elapsed, while the flesh continued to putrefy just as in the other set. Then using fine gauze as a covering for the jars, the result was the same. His mode of argument, therefore, was, that the cause of the formation of the maggots must be something that is kept away from the meat by the gauze. This something must be solid particles too big to go through the gauze, for air and fluids will readily pass through. Nor can there be any doubt as to what this something is, "for the blow-flies attracted by the odor of the meat, swarm around the vessel, and urged by a powerful, but in this case a misleading instinct, lay eggs out of which maggots are immediately hatched upon the gauze." *

These experiments were repeated with a great variety of substances, and with various modifications, but the results were uniformly the same, and so far as they went they carried conviction; but it must be remembered that they disproved spontaneous generation only for the special cases under consideration. The presumption, however, that all instances of the supposed origin of life from dead or inorganic matter might be in a similar manner explained, by the introduction in some way of living germs, rapidly gained ground, and was enunciated by Redi himself as at least probable. He even suggested that in this way we might explain the generation of the entozoa, or internal parasites of animal bodies. Redi was followed by Swammerdam** and Vallisnieri,*** who repeated his experiments, and the combined

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* HUXLEY: *ibidem.*

** JOHANNES: a Dutch physician and entomologist, 1637-1681. He was one of the earliest to make dissections of the human body. He published a number of entomological works. His "History of Insects" was claimed by Boerhaave, his editor, to be incomparably superior to anything that had preceded it. An English translation was published in 1758.

*** ANTONIO: an Italian physician and naturalist, 1661-1730. He studied medicine under Malpighi, and was subsequently Professor in the University

result of their writings was to entirely subvert the belief in the spontaneous generation of insects and all animals of a higher organization. Since their day no one of any scientific pretensions has ventured to propound this theory for any species of animal life with a high grade of organization.*

The discovery of the mammalian egg which dates from 1673, by De Graaf, of Delft, in Holland, and the full history of its mode of development which was closed by Von Baer, in 1827, threw a flood of light upon the general question of generation, and stripped it of the mystery which hitherto had been carelessly supposed to surround it in the highest orders of animals, especially in man himself. Its bearing, too, upon our subject is obvious. In this way "spontaneous generation lost its rank as a great natural division of the reproductive function; and came to be regarded as an exceptional phenomenon, confined to a very few species whose existence could not be accounted for in the ordinary way. Its territory was narrowed exactly in proportion as the knowledge of natural history advanced; and it became reduced almost exclusively to the class of animals known as *entozoa* or internal parasites."**

These are organisms, some of them microscopic in size, that live within and prey upon the bodies of other animals. They are found in special habitats or organs, and each species of animal has its own particular parasite. Thus, confining our illustrations to a few only of those met with in the human body, we may notice the different varieties of solid and hollow worms (*cestoidea* and *cœlelmintha*) that infest different portions of the alimentary canal; the *trichina spiralis* that dwells in muscle;

at Padua, and was especially celebrated for his researches into the various systems of generation. His works were published in three volumes folio, at Venice, 1733.

* Mr. Crosse's electrical spiders (*Sequel to Vestiges of Creation*), a kind of "microscopic porcupine," which he asserts were developed in a solution of silicate of potash, through which the constant galvanic current was continuously passed for two years, are unworthy the dignity of a serious refutation. A writer in the *Edinburgh Magazine*, April, 1857, humorously depicts this, and very appropriately too, as a most singular case of delusion.

** Dalton: *loco citato*, p. 121

the *strongylus gigas* that makes its abode only in the hilum of the kidney; then there are others peculiar to the brain, the liver, cellular tissue, etc. These creatures long puzzled and completely defied the naturalists in their efforts to explain the mode of their origin, and it is a curious study now to look at the shifting opinions which from time to time have been entertained regarding them. To illustrate: Linnæus, the celebrated naturalist, thought that the internal parasites were terrestrial or aquatic animals that had been swallowed with the food or drink. Bremser and Rudolphi, after twelve years of research, disproved this by showing that there was nothing in common in organization between such parasites and any known species. Boërhaave suggested that there was some metamorphosis or monstrous growth that occurred in them in their new and unaccustomed habitats. This was a leaning toward the truth, for we do find remarkable changes in successive stages of development, but the error was in the starting point.

Without dwelling further upon their opinions or without an attempt to detail the progress of the study, it is sufficient for my purpose to say that at last all these parasites were found to come from eggs, and in turn to produce young by sexual generation.* Years upon years of the closest investigation were necessary to complete this study, and the nature of the difficulties to be contended with were such that it seemed almost impossible to overcome them. This is well illustrated by the cysticerci, the intermediate stage or larval forms in the development of the tapeworms. They live in a closed cyst in the solid tissues, and they are absolutely sexless and unprovided with generative apparatus. To connect them, then, with the mature parasite, which lives in the alimentary canal alone, was a difficult task. The painstaking

* As late as 1858, Pouchet, the uncompromising advocate of the theory of spontaneous generation, questioned the truth of these discoveries in the generation of parasites. Says the writer in the *Edinburgh Review* (*loc. cit.*), "like a true Frenchman of the feebler sort he says, "*tant pis pour les faits!*" and rejects the facts which reject his hypothesis. He doubts the truth of these discoveries, "the monopoly of which," he naively says, "has by a singular anomaly belonged to foreigners." This reminds one of the pious patriotism of Lamartine, who said that when God has a noble idea to vouchsafe to mankind He always puts it first into the brain of a Frenchman.

labors of the helminthologists finally determined the mode of their origin, and completed the record of their natural history, by showing that, for the full round of their development two animals are necessary. The second of these usually stands to the first in the relation of prey or food. The mature parasite lays eggs in the alimentary canal of the first animal. These ova are swept out with the alvine discharges, and through the medium of surface water or herbage, some of them find their way into the alimentary canal of the second animal. Here the ova find conditions favorable to the first stage of their development, and they are now provided with a boring apparatus by which they make their way through the walls of the canal, and travel long distances, finally to ensconce themselves in the solid tissues where they become encysted. The second animal being killed, its flesh is eaten by the first. The cyst wall is digested, and the cysticercus, thus freed from its environment, now finds the appropriate nidus for the final stage in its development. Thus each tænia has its own cysticercus whose distinctive characteristics can be recognized under the microscope, and furthermore the tænia, peculiar to one species of animals, is never found infesting any other species.*

One can never sufficiently admire the splendid patience of such men as Diezing, Kuchenmeister, Haubner, Von Siebold, Leuckart, Van Beneden and others, who almost literally devoted their lives to these studies. The details of their experiments, both on man and on the lower animals, and their cautious, long-continued, and at times unpromising researches, form one of the most entertaining as well as instructive chapters in the whole record of natural history study.† These labors, it is true, were

* Van Beneden's little book, *Animal Parasites and Messmates*, published since this lecture was given, furnishes for the English reader an excellent account of the development and migrations of these entozoa. New York: D. Appleton & Co., 1876.

† An amusing illustration of the precision of the results obtained by these investigators is found in the well-known narrative of Van Beneden* in his monograph upon Intestinal Worms. For the purpose of illustrating the

* Van Beneden: *Mémoire sur les Vers Intestinaux*, Paris, 1858, p. 155, and *Animal Parasites and Messmates*, pp. 71 and 222.

not completed during the epoch under consideration, but during it, the presumption was clearly established that on fuller investigation a solution would be found of all cases that had hitherto baffled detection. The latest of these investigations worthy of note are those of Leuckart (1856–7) and Virchow (1858) upon the *trichina spiralis*; and they have confirmed in a very positive manner the opinion just stated, for it is a reasonable and almost universally admitted canon in scientific study that it is more probable that a law which is known to be without exception in phenomena, which we can clearly trace, extends to similar phenomena not yet fully explained, rather than that a new law should now come into play. This, of course, does not exclude the possibility of a new law, and the true scientist will cheerfully accept such a law, whenever by observation, comparison and experiment its correctness is established.

2*d Epoch* (*continued*). The other phase of the epoch under consideration relates solely to the origin of infusorial life. The microscope had been of great service in enabling scientists to account for the mode of generation in known animals, but with all this extension of knowledge it had also brought into view a new outlying territory which swarmed with animal life in numbers and kind before unsuspected. These are the infusoria first discovered by Leeuwenhoek in 1675 and called by him anima-

migration of parasites—a subject just then being established—he took with him from Louvain to Paris four pups which he had reared. Two of them he had fed upon the *cysticercus cellulosus* of the rabbit, the larval form of the *tænia serrata* of the dog. These pups he presented to a commission of scientists (Valenciennes, Milne Edwards, Quatrefages and Jules Haime) saying: In two of these dogs you will find not a single specimen of *tænia serrata*, in the other two you will find many; and furthermore, in this one you will find specimens in four different stages of development, while in that one you will find them only in three stages, and the number of specimens in this dog is much greater than in that one. The pups were then killed and his statements were proved to be absolutely correct. In one dog, however, some *tæniae cucumerinae* were found, and Van Beneden frankly owned he could not tell where they came from. Since then it has been discovered that they originate from an acarus, the *trichodectes*, which lives in the hair of dogs and which is infested by the scolex of this variety of tape-worm. The dog licking its hair swallows the acarus and thus infects itself very much in the manner in which a horse is infected with bots, by licking up the eggs of the *œstrus* or gad fly.

cules. In 1764 Wiesberg gave them the name which they now bear; this designation was made from the fact that they are always found in stagnant water and in infusions of both animal and vegetable materials after short standing. Subsequently they were studied by many observers, but it is to Ehrenberg* and Dujardin** that we are indebted for the most systematic descriptions of them, and their great works figure hundreds of varieties. The almost illimitable numbers, the great diversity of form and of organization, as well as the combined bulk of these microscopic beings are almost beyond conception. We now know that there are geological deposits of great size in different portions of the earth's crust that consist almost exclusively of the calcareous and siliceous shells of these minute beings. Indeed Ehrenberg himself regarded them "as forming by far the greatest number and perhaps also the largest mass of living animal organisms on the surface of the globe"† The rapidity of their development is something wonderful, and being also infinitesimal in size their mode of production was beyond the reach of the microscopes of the day, and it is no surprise to learn that the old doctrine of spontaneous generation was again invoked to account for their origin. This was done in 1748 by Needham†† and Buffon, who "led by certain theoretical considerations doubted the applicability of Redi's hypothesis to the infusorial animalcules, and Needham endeavored to bring the question to an experimental test."†† Taking the juices of meats which had been extracted at a high temperature he enclosed them in glass vials, also previously heated, corked them tightly and set them aside to cool. After a few

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* *Die Infusionsthierchen, als vollkommene Organismen.* Leipzig, 1838.

** *Histoire Naturelle des Zoophytes Infusoires.* Paris, 1841.

† DALTON: *loc. cit.*, p. 127.

†† HUXLEY: *loc. cit.*

††† JOHN TUBERVILLE: an English naturalist, 1713-1784. He was educated in the Roman Catholic faith and became a priest in that church, his life being mostly spent on the Continent. He was Director of the Academy of Maria Theresa, at Brussels. He devoted himself to scientific investigations in connection with his work in teaching, and published many papers. His principle work was a Treatise on Generation, published in French, the year previous to his death.

days he invariably found in the vials infusoria present in great and constantly increasing numbers. His argument then was that if they were produced from germs, the germs must exist either in the substance which had been boiled, the water in which it was boiled, or in the air enclosed in the vial. Now boiling destroys the vitality of all germs, hence no infusoria should be developed in his infusions. But they were invariably present in his vials and accordingly he assumed that they were generated by a reorganization of the dead animal matter. But as Huxley says, most eloquently, "the great tragedy of science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers, was played almost immediately for the benefit of Buffon and Needham."

The Abbe Spallanzini* thought that Needham's experiments had not been conducted with sufficient care and precision, as no account had been taken of the absolute temperature to which the flasks and infusions had been subjected, nor had the mouths of the flasks been absolutely closed from contact with the external air. He therefore took glass flasks partly filled with organic infusions, and after closing them by hermetically sealing up the necks, exposed them to the temperature of boiling water† for an

* LAZZARO: an Italian physician, born in the duchy of Modena, 1729, died at Pavia, 1799. He was educated at Bologna, where he subsequently became a Professor, and still later he was appointed to the Chair of Natural History in the University at Pavia. He was one of the most eminent men of his day, an honorary member of nearly all the learned societies of Europe, and universally held in the highest esteem. His scientific studies were principally in physiology, especially of the lower animals; and by these studies which have been incorporated into the text-books, his name is more familiar to the medical student of to-day than that of many other recent observers. He refused offers of Professorships in a number of the prominent institutions of the time, among them the *Jardin des Plantes* in Paris.

† Various expedients for ridding the flasks of any existing infusoria or germs have been adopted by different experimenters. Those usually employed are

1. *Calcination*—causing air to pass through red-hot tubes.
2. *Filtration*—passing air through any substance which shall catch and retain all foreign matters.
3. *Subsidence*—allowing the particles to settle by gravity.
4. *Expulsion*—driving out air and particles contained therein by hermetically sealing neck of flask while contents are in an active state of ebullition.

Two or even more of these methods may of course be combined in a single experiment. The great difficulty of excluding all germs from the flasks by

hour. Then setting them aside at ordinary temperatures, which are favorable to the generation of infusoria, even after the lapse of months not a trace of animal life could be found on breaking the flasks. This was in the year 1775. But Needham was not satisfied with these results, and with a show of reason claimed that such a prolonged boiling would destroy not only germs, but the germinative, or as he called it "vegetative force" of the infusion itself. Spallanzini easily disposed of this objection by showing that when the infusions were again exposed to the air, no matter how severe or prolonged the boiling to which they had been subjected, the infusoria reappeared. His experiments were made in great numbers, with different infusions, and were conducted with the utmost care and precision. The result seemed convincing and was in substance that whenever animalcules were found in infusions which had been exposed to great heat, they "are not produced there because their germs have resisted this temperature or because they have been generated spontaneously; but because new germs have been introduced into the infusion from the atmosphere after the boiling has ceased."

The naturalists of this period almost without exception acceded to these conclusions, doubt being entertained on a single point only. Oxygen had been discovered by Priestley in 1774, and its relation to the maintenance of life was for many years carefully studied by the physiologists. Now, might not the oxygen in the air of the flasks have been in some way altered by the high temperatures, and might not a renewal of oxygen be necessary to the development of life under any circumstances? This certainly seemed reasonable, and so it became necessary to repeat the experiments under conditions which would obviate these objections. This was done in 1836 and 1837 by Schultze and

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any method—even on the assumption that all those preëxisting within have been destroyed—may be appreciated when we recall the extremely minute size even of the fully developed parent animal. The *monas crepusculum* for instance is so small that eight millions of them would occupy a space no larger than a grain of mustard seed, and Prof. Owen has calculated that a single drop of water may contain five hundred millions of them. Such organisms would pass readily through imperceptible cracks or pores with changes in the temperature of the surrounding atmosphere.

Schwann, respectively. The first experimenter arranged his flasks with tubes bent at right angles and sealed to the stopper. To these tubes were attached a series of bulbs, which contained on one side anhydrous sulphuric acid and on the other a strong solution of caustic potash. Air was then by suction daily drawn into the flasks, passing in through the acid and emerging from the potash side. This process was continued for months (May to September) and no trace of infusoria, conferva or fungi was found in the fluids. Schwann's experiments varied from Schultze's in that he passed air in through a series of bent tubes, which were heated up to 600° F. The results, however, were the same, and in both cases it was proven that the air or oxygen had undergone no change. Thus it seemed clear that whenever life made its appearance in the infusions in closed flasks it was produced by germs introduced from without, and in the experiments under consideration, the germs in the atmosphere (if there were any) were destroyed by the acid and the calcination.

These experiments were accepted almost universally as demonstrative of the incorrectness of the theory of spontaneous generation and it may be said with propriety that within a few years subsequently the question was deemed to have been put to rest for all time. But a rigorous analysis of the evidence shows, as Prof. Huxley has very justly pointed out, that this conclusion is not warrantable. All that the experiments really proved was "that the treatment to which the contents of the flasks had been submitted had destroyed something that was essential to the development of life. This something might be solid, fluid or gaseous; that it consisted of germs remained only a hypothesis of more or less probability," and, no one, it must be remembered, had ever yet seen the germs. Helmholtz, in 1843, by his experiments narrowed this issue by showing that the interposition of a membrane between a putrefying (swarming with life) solution and one that is simple putrescible prevents the development of organisms in the latter. The cause of the development must therefore be something that cannot pass through the membrane. But gases and fluids can readily pass through, and hence it fol-

lows that it must be either a colloid or solid matter. Next in point of order Drs. Schroeder and Von Dusch* helped clear up up this point by showing that the simple exclusion of air from an infusion by a plug of cotton-wool prevented both fermentation and development of organisms; and finally Tyndall settled the matter definitely by showing that ordinary air is full of solid particles of matter, and that they are entirely strained out by filtration through the plug of wool. It only remains, therefore, to prove that among these particles are germs which, under appropriate conditions, are capable of being developed into animal life. "This," says Huxley, "was done by M. Pasteur, in those beautiful researches† which will ever render his name famous, and which, in spite of all attacks upon them, appear to me to be models of accurate experimentation and logical reasoning." In point of time, however, this demonstration was not made until the third or last epoch in the history of spontaneous generation.

3d Epoch. This dates from the year 1858, when the question—which by general consent had been considered as closed—was reopened Paris by Pouchet, the distinguished naturalist of Rouen. He sent a communication to the French Academy in which he declared that he had experimentally proven the truth of spontaneous generation, and in the following year he published his well-known work on the subject.‡ It may be well to note just here, as bearing upon the reliability of his evidence and arguments, that he was undoubtedly influenced by a motive, for in a preface by him to Pennetier's work on the origin of life, he says: "For all reflecting minds heterogenous production is a logical consequence of the appearance and ascending development of organized beings upon the globe."§ Furthermore, in the very opening paragraph of the preface to his own book, he uses this expression: "When *by meditation* it was evident to me that spontaneous generation was one of the means employed by

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* *Annal. de Chimie*, tome XLI., 1854, and *Chemical News*, Vol. V., 1862.

† PROF. TYNDALL, somewhat enthusiastically says, that his "labors in connexion with this subject may be fitly called immortal."—*Lancet*, February 12, 1876, p. 262.

‡ *Hétérogénie*; *ou Traité de la Generation spontanée, basé sur des nouvelles Expériences.* Paris, 1859.

§ Quoted by Dalton, *loc. cit.*

matter for the production of living beings, etc." This motive it is easy to see grew out of the tendency of the geological studies of the day, which show that the earliest remains of animal forms found in the earth's crust belong to the lower orders and gradually ascend in successive epochs to man. Hence it was an easy —I do not say legitimate—inference, that the higher orders had been gradually evolved out of the lower. And as the deepest and oldest geological strata show no organic remains it is a fair assumption that at some time in the great past there was no life on the globe; and hence another easy inference, that the first living beings which appeared were produced by spontaneous generation. This is the gist of the evolution theory and as an inducing motive in Pouchet's advocacy of spontaneous generation it is worthy of remembrance; for let me again remind you that such a question cannot in the nature of things be argued on the ground of probability, but must be determined solely by experimental evidence.

Pouchet further asserted that he had repeated Schultze's experiments with every possible precaution, but with totally different results. These assertions attracted much attention, and a few scientists sided with him, though the majority and among them the most of the leading physiologists opposed him. The question assumed different phases, and in January, 1860, it was made one of the prizes of the Academy. Pasteur, at this point, took up the matter and made the researches to which allusion has already been made. His first step was to ascertain whether in reality there are floating in the atmosphere spores of the microscopic fungi or germs of the infusoria, for by this time the question was confined almost exclusively to one point, viz: the atmosphere as the supposed source of the organic germs. For this purpose he passed air through a wad of gun cotton packed in a glass tube. Then dissolving the cotton in ether and alcohol he was enabled to gather in the deposit whatever floating particles had been caught upon the cotton during the forced passage of the air. Then examining these deposits with the microscope he found, besides the easily recognizable matters such as starch-granules,

hairs, coal-dust, etc., which are known to be floating in the atmosphere, numerous round or oval organized corpuscles, some of which "closely resemble the spores of the commonest moulds," and others "resemble the globular infusoria and are regarded as being the eggs of these small beings." "But, as to affirming," he says "that this particular one is a spore, or still more that it is a spore of a definite species, or that that corpuscle is the egg of an infusoria or of such a species, I do not believe that this is possible. I am content, as far as I am concerned, to affirm that these corpuscles are evidently organized." In the light of Lemaire's subsequent observations, which will soon be alluded to, it is demonstrated that this opinion of Pasteur's was correct.

Now assuming that such germs are floating in the atmosphere, Pasteur asserted that their number and variety would differ greatly in given volumes of air collected from different localities, and he even said in definite terms "that everywhere it was possible to detach a volume of air from the atmosphere which will contain neither egg nor spore, and will not produce generation in putrescible solutions." To determine this point he prepared a large number of flasks partly filled with solutions of sugar and yeast. After thorough boiling the flasks were hermetically sealed by drawing out the necks to a fine point. The flasks were then taken to different localities and opened by pinching off the necks. Air would rush in by reason of the partial vacuum which had been formed by the boiling of the contained fluid, and thus air from any locality could be gathered for experiment. In this way air was taken from the tops of high mountains, in the very midst of glaciers, from level, open plains in the country, from the streets of crowded cities, from cellars, etc. The result was that just in proportion to the distance from crowded cities, and the absence of disturbing currents in the atmosphere, the evidences of organic life diminished. Of twenty flasks which were opened on the "*Mer de Glace*," in the Alps, at an altitude of 6,000 feet, one only subsequently contained any trace of life. In another series of experiments flasks were filled in the cellars of the Observatory in Paris, where the temperature is almost uni-

form and the air is very still. The number containing organisms was very much less than in those filled in the garden of the same building. Pasteur predicted that if flasks could be opened and closed in deep cellars with absolutely no disturbance caused by the entrance of the operator there would be the same absence of vitality as in flasks which had been long exposed to red heat.* Later on he learned, what he had also predicted, that by simply turning the long neck of his flasks† downward they might be kept indefinitely without sealing or stoppers of cotton, and still no organisms would show themselves, for by this simple expedient the germs could not enter the flasks, gravity opposing.

On the other hand, Pouchet, assisted by MM. Joly and Musset, shortly afterward went over the same ground. He collected the solid particles from the air by means of an instrument which he called an aeroscope. This was a simple tube drawn out to a point. Air was passed in a jet through this and made to impinge upon a glass plate covered with some viscous substance. A pile of dust was thus caught, and then submitted to examination by the microscope. But strangely enough although he found plenty of foreign materials, like coal-dust, starch-granules, etc., not a trace of organic life, either in shape of spores or of germs. He was untiring in his researches. "He has," says M. Joly, "examined the dust which finds its way into the respiratory cavities in man and the lower animals; that which has been the accumulation of centuries in our Gothic cathedrals, and that which floats in the air of our public halls, our theatres, and our hospitals. He has crossed seas, climbed high moun-

* In an unexpected direction Prof. Tyndall by his recent experiments with closed boxes has practically verified this prediction, though the experiments were not made with cellar air. The similarity of the two cases, however, is apparent. *Vide postea*, p. 30.

† Many of Pasteur's flasks are still preserved in Paris, and by repeated examination have been found to remain unchanged. As late as November, 1874, M. Balard, in presenting to the Academy of Sciences a paper by M. Servel, detailing experiments which the writer held to be demonstrative of spontaneous generation, took occasion to say that he had just then examined in Pasteur's laboratory some of his *unsealed* flasks which contained blood that had been drawn more than eleven years previously, and in which, during all this time, no bacteria had appeared and no putrefaction had taken place.

tains, descended into the crater of Vesuvius and of Etna; he has penetrated even into the tombs of the Pharaohs and studied their crania blackened and dusty with the lapse of time."* It seems too bad after all this that he should not have been rewarded by occasionally finding germs, but his results were barren. Then he prepared a series of flasks with putrescible infusions as Pasteur had done and with the same end in view, of gathering air from different localities and learning whether subsequently organisms would develope in them. Now, holding as he did to the theory of spontaneous generation, he said, as the chemical constitution of the air is the same everywhere, we ought always to find such organisms wherever air, no matter from what part of the globe it may be taken, is brought into contact with putrescible solutions. And sure enough his flasks were found always to contain them.

Here then was a flat contradiction in the results obtained in each series of experiments by these two eminent observers. Each showed, at least to his own satisfaction, the fallacies in the experiments of the other, but the possibility of reconciliation seemed almost hopeless. It was therefore proposed to submit the case to a jury of experts to be selected by the Academy. The contestants availed themselves of this proposal, and a commission consisting of Flourens, Dumas, Brongiart, Milne Edwards and Balard was appointed in January, but did not begin its labors until June, 1864. Each contestant stated his propositions in definite and unmistakeable terms, and M. Joly, in his confidence, even went to the extent of saying, "If a single one of our flasks remains unchanged, we will acknowledge defeat." Pasteur appeared with sixty flasks and made his experiments. Pouchet and his confreres then declared that they were unwilling to abide by a decision on this series of experiments, and as the commission persisted in holding both sides to this series which

* *Les Generations spontanées*: Par JULES JAMIN, *Revue des deux Mondes*, Vol LIV., p. 431. Though somewhat argumentative, this article is a good resumé of the controversy before the French Academy, and the results on both sides are clearly set forth. A translation of the article was published in the *Methodist Quarterly Review* of Oct., 1865.

had been the principal cause of the controversy, Pouchet withdrew from the contest.* The Commission, however, continued its investigations, and in February of the next year they reported that the facts which were observed by M. Pasteur and contested by MM. Pouchet, Joly and Musset were of the most perfect exactitude.**

One point only needs now to be settled in order to complete the chain of evidence and render demonstration complete. Fortunately this was done before the rendition of the report. That is to collect and identify the germs from the atmosphere, and to propagate them, for it will be remembered that with a commendable prudence Pasteur had only stated his opinions as to the corpuscles and spores, which he had gathered in the manner already described. The heterogenists with force and with reason said, if such organic bodies are floating in the atmosphere, it is only fair that our opponents should show them to us. This was accomplished by Dr. Lemaire and Prof. Gratiolet in 1864. They condensed the moisture of the atmosphere in a wide open vessel,

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* "It is, perhaps, unfortunate," says Jules Jamin, "that the Commission held so stringently to the programme as to let slip the unique opportunity of a solution which was expected from it. But it is evidently clear that the heterogenists, however they may have colored their retreat, were self-condemned. If they had been sure of the fact, which they had solemnly undertaken to prove under penalty of acknowledgment of defeat, they would have persisted in proving it, for it would have been the triumph of their doctrine. It is doubtful causes only that are allowed to go by default."—*Revue des deux Mondes*, Vol. LIV, p. 438.

** En resumé, les faits observeés par M. Pasteur et contestés par MM. Pouchet, Joly et Musset, sont de la plus parfaite exactitude. Des liquers fermentescibles peuvent rester, soit au contact de l'air confiné, soit au contact de l'air souvent renouvelé, sans s'altérer, et quand sous l'influence de ce fluide il s'y développe des organismes vivants, ce n'est pas à ses éléments gaseux qûil faut attribuer ce développment mais à des particules solides dont on peut dépouiller par les moyens divers, ainsi que M. Pasteur l'avait affirmé. *Comptes Rendus*, vol. lx, p. 396.

Running through the *Comptes Rendus* from the year 1858 to 1865, the reader will find all the facts and reports upon this remarkable controversy, which, as Dr. Dalton remarks, may almost be said to have kept the Academy in a turmoil for some six or seven years, and which at times was so conducted as to provoke considerable bad feeling.

A good sketch also of Pasteur's experiments may be found in Schützenberger on Fermentations, Vol. xx, of the International Scientific Series, published by D Appleton & Co., New York.

which was surrounded by ice. Water was thus obtained from different localities. It was carefully enclosed in glass tubes and submitted to examination. The liquid thus condensed was at first, colorless, clear, and contained no living being. There were, however, "myriads of spherical roundish and fusiform spores, pale cells and semi transparent ovoid bodies," besides, of course, the foreign matters. "At the end of fifteen hours large numbers of living bacteria were found; in forty-eight hours vibrios and spirilla swarmed in abundance, and in three days monads, whose incubation is slower, were also present. *Just in proportion as this mass of life appeared, the spores and semi transparent corpuscles disappeared.*" These experiments, varied in many ways, even to the extent of sowing as seed the particles obtained from the air, and thus propagating infusorial life, were demonstrative of the actual existence of organic germs in the atmosphere. In this way the work of the Commission was materially aided, and the decision which they rendered was generally accepted as conclusive against spontaneous generation.

The heterogenists, however, even to-day do not accept these conclusions, and, although they grant that the usual mode of the development of infusorial life is from pre-existing germs, they claim that, under exceptional conditions, it may arise spontaneously. Foremost among these advocates, and conspicuous for his attainments, is Dr. H. Charlton Bastian, of London. His labors in this direction and his publications, both fugitive and systematic,[1] are familiar to you all. I shall, therefore, for lack of time attempt no description, not even a summary of his experiments, but this sketch would be incomplete without a reference to them. I would in no way underestimate their importance as contributions to our knowledge on the subject, but after examination of all the evidence which has been accessible to me, I am unable to see that his work has advanced the main question

1 *The Modes of the Origin of Lowest Organisms, including a discussion of the Experiments of M. Pasteur, and a Reply to some Statements by Profs. Huxley and Tyndall.* London, 1871.

The Beginnings of Life: being some Account of the Nature, Modes of Origin and Transformations of Lower Organisms. 2 Vols., London, 1872.

materially beyond the point where the French Academy left it. In some minor particulars, his work has been of great service. His experiments have been analysed very carefully, and in many cases repeated by Frankland,[1] Burdon Sanderson,[2] Sedgwick, Lionel Beale, Roberts,[3] Lankester,[4] Tyndall,[5] Huxley[6] and others, and numerous sources of error pointed out. Some of these authorities are believers in the possibility of spontaneous generation and later, on p. 25, I have quoted opinions from them, but no one of them, so far as I am aware, admits that Dr. Bastian's experiments are conclusive, or that spontaneous generation has ever yet been demonstrated. Some of them, too, have been very wrongly quoted by Dr. B. and other heterogenists as supporting their views. On the other hand there are still some who openly avow their belief in the doctrine, and I may mention here a few names that occur to me at this writing of men eminent for their scientific attainments—Mr. Wallace,[7] Prof. Huizinger,[8] of the University of Gröningen, Prof. Cantoni and others, of the University of Pavia, and Ernst Haeckel.[9] The latter, however, is a

(1) *Nature*, Vol. III, p. 225.

(2) *Ibidem*, Vol. IV, p. 377, and Vol. VIII, pp. 141, 181.

(3) Mr. Roberts was forced, by his own experiments, apparently very unwillingly, to believe in the possibility of spontaneous generation. Prof. Tyndall, in his recent paper before the Royal Society, (p. 29), has pointed out a minute error of detail which vitiated the results and led Mr. Roberts to his conclusions. For his experiments see *Philosphical Transactions* Vol. CLXIV., 1874.

(4) *Nature*, January 30, 1873, page 242, and Oct. 16, 1873, p. 505.

Lankester and Podes original experiments are reported in detail in *Proceedings Royal Soc.*, Vol. XXI., 1873.

(5) *Medical Times and Gazette*, Oct., 1870, page 406, and *The Lancet*, February 12, 1876, p. 202.

(6) *Nature*, Vol. II., p. 473.

(7) An elaborate review of Dr. Bastian's larger work was published by Mr. Wallace, in *Nature*, Aug. 8 and 15, 1872.

(8) *Pflüger's Archiv*, Vol. VII., p. 549. An advance summary by himself of the experiments detailed in this paper may be found in *Nature*, March 20, 1873, p. 380. See also comments on the same by J. Burdon Sanderson. *Ibidem*, Oct. 2, 1873, p. 478, and *Med. Times and Gazette*, Sept. 27, 1873, p. 364. Sanderson interprets these experiments differently from Dr. Bastian and Huizinger himself.

(9) Prof. E. Ray Lankester has published a lengthy abstract of Haeckel's opinions in *Nature*, March 2, 1871, p. 355.

supporter of this side of the case from purely theoretical considerations, for, although he concedes that, thus far there is no absolute proof of the theory, he holds that the difficulties in the way of ultimately establishing it are not only surmountable but less formidable than those that face the supporters of biogenesis.

I may sum up then, without further detail, the whole matter, by saying that there is no trustworthy evidence to-day that spontaneous generation has been demonstrated in a single instance. Even Huxley, who declares that if it were given him "to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions which it can no more see again than a man can recall his infancy, he should expect to be a witness of the evolution of living protaplasm from not living matter" says very significantly that, with this limitation, Redi's great doctrine of biogenesis seems to him victorious along the whole line at the present day. Authority*, however, cannot settle the question,

* Notwithstanding this admission, I venture to add here a few opinions which I have noted, without special search, in the course of my reading. Even if not decisive of the question at issue, such opinions are interesting.

Sir Wm. Thompson: "I am ready to adopt as an article of scientific faith, true through all space and through all time, that life proceeds from life and from nothing else." President's Address, British Association, 1874. *Nature*, Vol. IV, p. 269.

Haeckel: "Positive contradiction of the hypothesis of Archigenesis is impossible. Positive proof there is not yet since no one has yet seen any organism take origin except by parentage. * * * Either the *monera* were once for all at the beginning of organic life on the earth produced by Archigenesis, * * * or in the course of the earth's history they have been produced by recurring acts of Archigenesis, and in this case there is no reason why this process should not occur at the present time." *Nature*, March 2, 1871, p. 356.

J. Burdon Sanderson: "I do not hold that spontaneous generation is impossible. I do not regard heterogenists as scientific heretics. All I say is, that up to the present moment I am not aware of any proof that they are right." *Nature*, Oct. 2, 1873, p. 479.

M. Flourens: "So long as my opinion was not formed, I had nothing to say. Now that it is formed I will express it. Pasteur's experiments are decisive. If spontaneous generation is a reality what is necessary to produce animalcules? Air and putrescible fluids. Now M. Pasteur puts air and putrescible liquids together and nothing comes of it. There is then no spontaneous

and in entering this Scotch verdict of *not proven*, I simply accept what I believe to be the correct interpretation of the best attainable evidence in the present state of our science. If new evidence can be adduced which is subversive of this conclusion, we must accept it without regard to our predilections or beliefs. To reject the theory on such considerations is contrary to the scientific method, and it is by this method alone that experimental evidence should be interpreted.

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generation. To doubt longer is not to comprehend the question."—*Revue des deux Mondes*, Vol. LIV, 1864, p. 441.

PASTEUR; "This conclusion which I have already formulated is unassailable. *In the present state of science the hypothesis of spontaneous generation is a chimera.*" Translated from a letter to Prof. Tyndall, dated Paris, February 8, 1876. *The Lancet*, Feb. 19, 1876, p. 296.

HUXLEY; "If in the present state of science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment doubtful. But though I cannot express this conviction of mine too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as Abiogenesis ever has taken place in the past or ever will take place in the future. * * * All I feel justified in affirming is that I see no reason for believing that the feat has been performed yet."—President's Address, British Association, 1870. *Nature*, Sept. 15, 1870, p. 403.

BASTIAN: "On account of this *à priori* probability and in the face of this evidence, I am, therefore, content, and, as I think, justified in believing that living things may and do arise *de novo*." Dr. B's. views have been often summarized in his prolific writings, but I know of no more concise expression of them than the above, from a closing paragraph of a series of papers "on the Heterogeneous Evolution of Living Things."—*Nature*, July 14, 1870, p. 228.

LIONEL S. BEALE: "I confess to being an opponent of the doctrine, but simply because I cannot admit that the evidence yet adduced is at all convincing. * * * The fact of *à priori* arguments having been so very much dwelt upon, makes me think that the mind of the experimenter may have been to some extent prejudiced (prepossessed) in favor of the doctrine he seeks to support by new facts, and in this way they are calculated to excite in my mind, however much I may resist, a doubt whether the inferences which have been arrived at really have been deduced from facts of observation and experiment *only*."—*Nature*, July 28, 1870, p. 254.

SMITH, WORTHINGTON G.: "It seems to me rational enough to suppose that unicellular bodies and objects of the lowest possible organization may be heterogeneously produced from the inorganic world."—*Nature*, August 4, 1870, p. 276.

VALENTIN: Prof. Physiology, Univ. Bern. "On the whole, the hypothesis of a spontaneous generation of plants or animals can only be regarded as a

An important, if indeed it be not the pivotal point in the recent discussions of this question, is the degree of heat to which vegetable and animal germs can be submitted without destroying their vitality. On this point there is great discrepancy of opinion. To admit that 212° F. is sufficient, is to destroy absolutely, as Pouchet pointed out, the validity of Spallanzini's and Schultze's experiments. I allude to this, not for argument's sake, which is foreign to a historical review of the question, but merely to enable me to state several interesting facts. Prof. Jeffries Wyman*, of Harvard College, found infusoria in infusions that had been boiled four hours, but he found none after five or six hours boiling.

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kind of superstition which is constantly receding before the advance of the natural sciences."—*Text-Book of Physiology*. London, 1853, p. 624.

Pouchet, in his *Hétérogenie*, previously alluded to, (p. 17) deliberately quotes Valentin as a supporter of his (Pouchet's) views, whereas he is an uncompromising opponent of them.

Van Beneden: "It is evident to all those who place facts above hypotheses and prejudices, that spontaneous generation, * * * does not exist, at least if we only consider the present epoch."—*Animal Parasites and Messmates*, p. 106.

Carpenter, W. B.: "The doctrine of 'spontaneous generation' cannot now be said to have any claim whatever to be received as even a possible hypothesis."—*Principles of Physiology, Genl. and Comp.*, 3d edition, Philadelphia, 1851, p. 866.

Huizinger: "Under the above described circumstances [i. e. his experiments] Bacteria can arise without pre-existing germs. Not in any single case have I seen any other organisms than Bacteria—never fungi."—*Nature*, March 30, 1873, p. 381.

James Samuelson: "If the believers in spontaneous generation still insist that their hypothesis has not been refuted and that, assuming my observations to be correct, their view of the case has not been fully disproved, I am not prepared to deny this. But, on the other hand, I must be permitted to retort that their experiments have only proved, so far, their inability, notwithstanding all their precautions, to exclude invisible germs from their infusions."—*Med. Times* and *Gazette*, Sept. 24, 1870, p. 376.

Tyndall: "As far as inquiry has hitherto penetrated, life has never been proved to appear independently of antecedent life."—*Nature*, February 3, 1876, page 269.

* Prof. Wyman's experiments are justly deemed among the most valuable contributions ever made to this subject. They are recorded in the *Amer. Jour. of Sci. and Arts*, Vol. XXXIV, 1862, p. 79, and Vol. XLIV, 1867, p. 152.

Professor Cantoni,* of the University of Pavia, by means of a Papin's digester, carried the temperature of his flasks to 110°–117° Centigrade, and yet vibrios in large number, were produced in two days. And Dr. Bastian has repeatedly noted a temperature ranging from 148° to 150° C. (equal to 312 F.) to which his infusions have been submitted for a brief period, and yet living matter has soon developed. On the other hand, some of the living infusoria will flourish in temperatures below the freezing point. They may even be dried and kept for years and then by the application of moisture they will revive and resume active motion. The truth seems to be that the germs, at least, are of well nigh inextinguishable** vitality, but it is difficult, if not impossible, to secure consent as to the precise limits of temperature, wet and dry, and other conditions under which such vitality may be retained. Until these points can be determined it seems almost hopeless to expect a solution which will command assent from both parties to the controversy.

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* *Gaz. Med. Ital. Lombard.*, Ser. VI., Vol. 1, 1868. It is a significant fact in this connection that Cantoni's, Wyman's, Huizinger's, and others' experiments have been interpreted by the adherents of opposite sides of the question as substantiating their own view of the matter.

** "The tenacity of life [of the Rotiferæ] is one of the most extraordinary phenomena. Their resistance to cold is something marvellous, and we don't even know where it stops; the lowest temperature that we can obtain in our laboratories does not seem to have any effect upon them * * * I have sometimes removed them quickly from the freezing apparatus and thrown them into a stove heated to 176° Fahr. * * * In this two-fold test and formidable transition from cold to heat, these microzoa passed rapidly through a change of 216° Fahr. without being in the least inconvenienced by it." Pouchet. *The Universe*, p. 56. 2d Ed., 1871.

Rudolphi long ago learned that the entozoa in frozen fish when thawed out resumed their customary activity. Frankland has recently described ice-fleas, which flourish in the glaciers of the Alps, at a temperature constantly below freezing point.

Mr. Bauer kept the *vibrio tritici*—a parasite of wheat—dried for seven years, and on moistening them with water they resumed their active motions.

M. Balbiani in 1857 "observed a drop of water on a plate of glass in which were living colpods. When the water was evaporated each became encysted and dormant in its envelope. The plate was moistened again in 1864, when every colpod was observed to come out from its shell and promptly resume its vital functions, which had been interrupted by seven years of sleep."—*Revue des deux Mondes*, Vol. LIV., 1864, p. 439.

Though not pertinent, strictly speaking, to a historical sketch like this of the doctrine of spontaneous generation, it is interesting to know what are the organisms whose germs can withstand such savage treatment as that described, and whose mode of generation is involved in any obscurity. They are only the very lowest orders of life known, and by common consent the question is limited to the *Monas, Vibrio, Spirilla and Bacterium.* The last three are generally conceded to belong to the vegetable world. At all events, they stand upon the extremest limits of that debatable ground in which spontaneous generation, if it is ever shown to be a reality, must be found. The higher orders, comprising almost the entire class of infusoria, are now known to reproduce themselves by true sexual generation.*

The last, but by no means most unimportant contribution to this subject is by Prof. Tyndall; and I cannot close this sketch without alluding to his work, although it was not undertaken for the purpose of solving or attempting to solve the problem of spontaneous generation. His investigations† were a continuation, practically, of his former experiments on floating particles in the air (to which allusion has already been made, p. 17), and were for the purpose of supplying direct evidence to connect

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* The observations of Stein, Englemann, Balbiani and others have clearly established this fact for a large share of the infusoria. Their labors have been supplemented and their results corroborated by the elaborate studies of Ernst Eberhard. An abstract of his work may be found in *Quar. Jour. Micros. Sci.* New Series, Vol, VIII, p. 155. See DALTON, *loc. cit.* for reference to Stein and others.

† *Medical Times and Gazette*, January 29, 1876. Also the *Lancet*, same date. The title of his paper is "On the Optical Deportment of the Atmosphere in Reference to the Phenomena of Putrefaction and Infection." An abstract by the author himself was published in *Nature*, February 3, 1876.

Since this lecture was given Dr. Bastian has sharply criticised both Tyndall's experiments and his deductions. In turn Prof. Tyndall has made a rejoinder of equal positiveness and severity. Dr. B. entitles Tyndall's paper "a new attempt to establish the truth of the germ theory," and then unsparingly attacks not the germ theory of disease but the doctrine of biogenesis—questions which are far from being identical. A most dispassionate review of Dr. Bastian's position in this controversy may be found in the *Popular Science Review*, 1876, in a paper by Rev. W. H. Dallinger, V.P.R.M.S. This paper contains also some valuable discoveries made by the author and Dr. Drysdale.

zymotic changes (putrefaction) with the presence of such particles. The experiments show that there are particles of matter in the air which are ultra-microscopic in size, and yet their presence can be made evident by their power of refracting light, and *secondly*, whenever air deprived of such particles is brought in contact with putrescible solutions no putrefaction occurs, but that it invariably occurs wherever the air does contain such particles. An air tight box with a glass side and windows in the ends had sealed to its bottom twelve test tubes with their mouths upward and projecting inside the box. In the top was an India rubber stuffing box through which passed a glass tube by means of which infusions could be dropped into the test tubes. The whole interior was smeared with glycerine and the apparatus allowed to stand a number of days. By subsidence, then, all foreign matter was caught and retained on the bottom. Now when the electric beam is passed through the box, the space inside appears perfectly dark, and the freedom from dust is thus proven, for, so long as there are floating particles, the track of the beam can be detected. Then organic solutions of different sorts were dropped into the tubes and boiled from below (the tubes being made to project from the bottom of the box, for this purpose) for a space only of five minutes. In no single instance, except where the cause of the failure was obvious, did any turbidity occur in the solution, nor was organic life (bacteria) found after even a lapse of weeks and of months. The conclusion thus reached by Prof. Tyndall is that the power of scattering light and of developing bacterial life by the atmosphere go hand in hand, and both are dependent upon the presence of particles which, even by the highest powers of the microscope, we cannot detect. As bacterial life is regarded as necessary to putrefaction, incidentally, therefore, spontaneous generation is held to be negatived by these experiments. This deduction will be accepted or rejected according as one is inclined to side with one or the other parties to the controversy. I cannot refrain from expressing my own opinion, that not only are the experiments of the utmost exactness, but also that already they have led to results which will contribute very materially to the ultimate solution of this problem.